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Correspondence

Duplex Ultrasound and Angiography

Sir,

I read with interest and growing concern the paper by Pemberton *et al.* (*Eur J Vasc Endovasc Surg* 1996; **12**: 452–454) regarding the use of duplex ultrasound as a replacement for angiography prior to infrainguinal bypass surgery. The primary cause for my concern about this paper, and others like it, is the implication that the risk of a diagnostic procedure outweighs its diagnostic accuracy, and more importantly its ability to predict the outcome of the proposed surgical procedure.

It is accepted that the primary cause of symptoms is impaired haemodynamics of the lower limb arterial system and the functional effect of occlusive disease is secondary to its anatomical severity. However, to infer functional impairment from anatomical measurements of individual stenoses made at angiography or ultrasound is prone to a high degree of error for a number of reasons: (a) the relationship between diameter or area reduction and haemodynamic effect is very non-linear,[1–3] (b) the degree of asymmetry of a stenosis is a significant factor that is not taken into account,[4] and (c) all measurements involve a degree of operator error.[5] In addition, arterial disease is usually multi-focal and there is currently no way to accurately predict the functional effect of a series of partial occlusions using measures of stenosis geometry alone. It is often said that angiography is only an anatomical assessment. This is not entirely true, as considerable subjective functional information is available at the time the angiogram is performed (e.g. relative flow rates, degree of collateral development) in addition to objective functional measurements (e.g. arterial pressure and the potential to measure absolute mean and pulsatile flow rates[6–9]). In practice it seems that the overall severity of the disease is assessed using the patient's symptoms and non-invasive tests (e.g. indirect pressures and walking distance), while imaging is used more to assess the relative distribution of significant lesions, so it is necessary to image the entire arterial tree of the lower limb. The profunda femoris artery (PFA) is an important vessel to assess in a symptomatic patient, as conservative treatment of superficial femoral artery (SFA) occlusion relies on a satisfactory profunda collateral circulation. In addition, both open and percutaneous profundaplasty are effective and low risk procedures that have a place in the surgical management of some patients.[10–13] Angiography, therefore, has the distinct advantage over duplex ultrasound in that it is able to clearly demonstrate the whole PFA and the collateral circulation.

The ideal preoperative assessment strategy would (a) reliably determine which patients will benefit from intervention and (b) reliably predict the outcome of the proposed operation for each patient. As it seems neither conventional angiographic nor duplex ultrasound imaging can even accurately predict the severity of a patient's symptoms,[14] it seems illogical that one should be compared with the other as the ideal preoperative assessment. It is clear that duplex ultrasound has a major role in preoperative assessment, but the conclusion should not be that ultrasound is as good as angiography . . . rather that ultrasound and angiography are equally bad! Assuming the risk and cost of an inappropriate vascular reconstruction far outweighs that of an angiogram, the emphasis should focus on the predictive accuracy of the preoperative imaging procedures using objective measures of functional outcome. Neither angiography nor duplex ultrasound can yet be regarded as "gold standards" in this respect: instead they should be considered to be complementary techniques that still have considerable scope for improvement.

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Vein Graft Surveillance

Sir,

We have read the review article by Golledge *et al.*[1] with great interest and some concern. The conclusions drawn by the authors seem to have widespread consequences and should be given some consideration before accepting them, especially as this review article has been quoted in literature summaries. Concerning the statistical methods used: in the discussion of their article the authors concede that meta-analysis is not really the correct tool for retrospective data. Why did they not use the method of calculating the difference of proportions between the two series instead of the Chi-squared test, where the former allows for the calculation of confidence intervals as well as *p*-values. In our small evaluation of the data we used the 99% confidence interval, while the authors used the 0.01 level for statistical significance.[2]

Furthermore, one should realise that the article deals with a historical control group. The median year of publication of the articles without surveillance is 1980 versus 1989 for articles with surveillance. During this time span something has certainly changed in vascular surgery. In our own institution, for instance, we have become more aggressive in performing arterial reconstructions. We are now performing bypasses in patients who we would not have treated 10 years ago. Why is the total occlusion rate higher in the series without bypass surveillance? This could have something to do with the more frequent use of intraoperative control measures in the later series with bypass surveillance. This is another sign of change over time. The term critical ischaemia is also used by the authors for the control series, although this term was not defined in the literature until 1986.[3] Why does a total occlusion rate of 27% in the series of articles without surveillance lead to an amputation rate of merely 13%? In other words, less than 50% of the legs with occluded bypasses were amputated. If one compares this with the 70–80% amputation rate when a bypass performed for critical ischaemia occludes, there clearly is a discrepancy.[4] We suspect that the series without surveillance were not all performed for critical ischaemia as the series with surveillance seem to have been.

There is another obvious difference between the two series of articles, namely the reporting rates for amputation. There could be several reasons for this difference: either the amputation rates in the surveillance series were very low, and therefore considered not worth mentioning, or there was no change in the amputation rate, leading to a possible bias of not publishing it. Moreover, the reporting rates for amputations have been counted wrongly in the review, and should be six of 17 for the publications with surveillance and 21 of 26 without surveillance (Tables 2 and 3 in Golledge *et al.*). These rates are clearly different in the two groups of articles: the difference of the proportions is 0.45, with a 99% confidence interval between 0.096 and 0.814, $p = 0.007$.

The article by Berkowitz and Greenstein has been quoted wrongly.[5] Although these colleagues do have an elaborate surveillance program, duplex sonography is not mentioned in their publication. There is a second incorrect quotation in the article. The figures in the publication by Thompson *et al.*[6] are different from those appearing in Tables 1 and 2 in Golledge *et al.* In the article 206 femorodistal reconstructions are reported without mentioning amputations.

There is another point to make. If one studies the endpoint of limb salvage, why include data from publications which do not give information about it? If one performs a statistical analysis of the total occlusion rates based on these articles only, another picture arises: without surveillance 804 occlusions in 2957 bypasses (27%), with surveillance 103 occlusions in 452 bypasses (23%). The 99% confidence interval for this difference of the proportions of 4% ranges between -1 and 10%; $p = 0.056$. The difference is not nearly as significant as shown in the review. The amputation rates are 355/2957 (12%) for the articles without and 69/452 (15%) with surveillance ($p = 0.062$). The 99% confidence interval for the difference of the proportions lies between -1 and 8%. Is there a tendency, perhaps, that duplex